	Wolf's Relative Numbers (Zürich). 1886	1887	Variation in Magnetic Declination (Milan). 1886	1887
			′	′
January	28·4	13·1	4·07	3·71
February	23·6	15·7	4·91	3·69
March	61·8	2·7	8·61	6·99
April	45·9	7·5	9·89	9·33
May	29·0	17·2	9·06	9·30
June	25·7	16·3	8·37	9·55
July	32·9	26·2	9·58	10·25
August	19·0	21·1	8·17	9·07
September	17·1	6·9	7·61	6·08
October	9·5	5·4	6·33	6·03
November	0·0	4·5	2·48	3·07
December	15·1	20·5	1·61	2·23
Mean	25·7	13·1	6·72	6·61

The fluctuations in the numbers and dimensions of the prominences have not been so great as for the spots, but the prominences likewise showed a maximum in July and a decline afterwards. The highest prominence observed by Prof. Tacchini during the year was on July 2, 2¼′ in height. Both faculæ and prominences failed to show a depression similar to that so conspicuous in November in the numbers of the spots, or the revival these displayed in December, the faculæ thus according in their behaviour rather with the prominences than with the spots. The following figures, given by the Rev. S. J. Perry in the *Observatory* for February 1888, show the general decline in prominence activity during 1887, as compared with 1886:—

	Mean Height of Chromosphere.	Mean Height of Prominences.	Mean Extent of Prominence Arc.
	″	″	°
1886	8·05	24·78	13·26
1887	8·13	23·86	9·29

A NEW COMET.—A comet was discovered by Sawerthal on February 18. It was observed at Cape Town, February 18, 14h. 32·5m., in R.A. 19h. 11m. 32·5s., and N.P.D. 146° 3′ 44″. Daily motion, R.A. + 7m.; N.P.D. − 1° 15′. Its physical appearance was as follows:—It was about the seventh magnitude, had a well-defined nucleus, and a tail a degree in length. It was visible to the naked eye.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *MARCH* 4–10.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on March 4

Sun rises, 6h. 40m.; souths, 12h. 11m. 46·0s.; sets, 17h. 44m.: right asc. on meridian, 23h. 2·5m.; decl. 6° 9′ S. Sidereal Time at Sunset, 4h. 36m.

Moon (Last Quarter on March 5, 3h.) rises, 0h. 26m.; souths, 5h. 14m.; sets, 9h. 54m.: right asc. on meridian, 16h. 3·8m.; decl. 15° 53′ S.

Planet.	Rises.		Souths.		Sets.		Right asc. and declination on meridian.			
	h.	m.	h.	m.	h.	m.	h.	m.	°	′
Mercury	6	14	12	1	17	48	22	51·6	3	21 S.
Venus	5	35	10	5	14	35	20	55·6	17	37 S.
Mars	21	49*	3	7	8	25	13	56·3	9	2 S.
Jupiter	1	14	5	27	9	40	16	16·5	20	22 S.
Saturn	13	20	21	18	5	16*	8	10·1	20	40 N.
Uranus	20	39*	2	13	7	47	13	1·8	5	51 S.
Neptune	9	11	16	51	0	31*	3	42·4	17	59 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

March.	Star.	Mag.	Disap.		Reap.		Corresponding angles from vertex to right for inverted image.	
			h.	m.	h.	m.	°	°
4	49 Libræ	5½	0	0	0	30	334	274
6	B.A.C. 6098	6	2	28	3	25	72	200

March.	h.	
4	11	Jupiter in conjunction with and 3° 47′ south of the Moon.
4	14	Mars stationary.
9	22	Venus in conjunction with and 0° 17′ north of the Moon.

Saturn, March 4.—Outer major axis of outer ring = 44″·8; outer minor axis of outer ring = 16″·0; southern surface visible.

Variable Stars.

Star.	R.A.		Decl.						
	h.	m.	°	′			h.	m.	
T Arietis	2	42·1	17	3 N.	Mar.	8,			*M*
Algol	3	0·9	40	31 N.	,,	4,	0	1	*m*
					,,	6,	20	50	*m*
R Persei	3	22·9	35	17 N.	,,	5,			*M*
λ Tauri	3	54·5	12	10 N.	,,	7,	0	20	*m*
					,,	10,	23	12	*m*
ζ Geminorum	6	57·5	20	44 N.	,,	4,	22	0	*M*
					,,	10,	2	0	*m*
R Canis Majoris	7	14·5	16	12 S.	,,	9,	21	25	*m*
S Cancri	8	37·5	19	26 N.	,,	6,	20	59	*m*
δ Libræ	14	55·0	8	4 S.	,,	7,	1	6	*m*
U Coronæ	15	13·6	32	3 N.	,,	10,	4	7	*m*
U Ophiuchi	17	10·9	1	20 N.	,,	5,	1	28	*m*
X Sagittarii	17	40·5	27	47 S.	,,	4,	3	0	*M*
β Lyræ	18	46·0	33	14 N.	,,	7,	22	0	*M*
U Aquilæ	19	23·3	7	16 S.	,,	10,	5	0	*m*
η Aquilæ	19	46·8	0	43 N.	,,	9,	5	0	*m*
Y Cygni	20	47·6	34	14 N.	,,	4,	19	11	*m*
					,,	7,	19	5	*m*
W Cygni	21	31·8	44	53 N.	,,	5,			*m*
δ Cephei	22	25·0	57	51 N.	,,	10,	22	0	*m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
	°	°	
From Coma Berenices	190	26 N.	March 8.
Near η Libræ	234	17 S.	Swift. March 7.
,, γ Herculis	244	16 N.	Very swift. Mar. 7.

THE RELATIONS BETWEEN GEOLOGY AND THE BIOLOGICAL SCIENCES.[1]

II.

IN the remarks which I have hitherto made, I have confined myself to the purely biological aspects of palæontology. As astronomy exhibits to us the orderly working of physical and chemical laws in other and far distant orbs, so palæontology presents us with the biological phenomena of many and widely-separated periods.

But besides the biological, there are two other aspects in which fossils may be viewed; and in these aspects their relations are almost entirely with zoological science. It is the recognition of this fact which prevents the geologist from acquiescing with the claims of biologists to treat palæontology as nothing more than a branch of their own science.

The assemblage of fossils found in a particular deposit furnishes us with the most valuable evidence concerning the conditions—such as salinity of water, depth, temperature, pressure, &c.,—under which the deposit must have been formed. And, again, in the changes which the materials of fossils can be shown to have undergone we have very accurate data for determining the succession of processes to which the materials of the deposit must have been subjected since their original accumulation.

It is true that this evidence of fossils concerning the conditions under which deposits have been formed, is of a kind which has been sadly misread in the past. Until the study of deposits which are being formed at the present day was taken up in a systematic manner, it was almost hopeless to avoid numerous sources of error; but at the present day the advantages accruing to geology from the results of deep-sea researches, are at least as great as those which by the same means have been conferred upon biology.

It is almost needless to call attention to the fact that there are vast masses of rock, including most of the calcareous and carbonaceous, and many of the siliceous and ferruginous types, of which the materials have been accumulated entirely by the agency of living organisms; it is impossible to study the petrology of such deposits without an acquaintance with the nature and functions of the organisms by which they were formed. But, even in the case of many arenaceous and argillaceous deposits, living organisms have played a very important part in their formation. Much of the materials of such rocks can be shown to have been used in building the coverings of organisms, to have filled up their dead shells, or to have been passed through their bodies, before being finally buried under other masses. Rocks destitute of all traces of the solid parts of animals often abound with worm-tracks, burrows, or casts.

---

[1] Address to the Geological Society by the President, Prof. John W. Judd, F.R.S., at the Anniversary Meeting, on February 17. Continued from p. 404.

The study of the processes by which similar rock-masses are being formed at the present day constitutes the only safe guide to us in interpreting the structures presented by ancient rock-masses. Geologists look forward with much interest to the publication of those volumes of the *Challenger* Reports, in which Mr. Murray and M. Renard will deal with these important questions.

We may especially call attention to two classes of errors that have had much to do with the false conclusions which have been arrived at concerning the conditions under which various deposits have been formed in past geological times.

In the first place, it has been tacitly assumed that all marine organisms which come from regions bordering the equator must necessarily have lived under tropical conditions. It would be quite as reasonable to treat the mosses and dwarf willows which border the eternal snows of Chimborazo and Kilima-Njaro as tropical plants. Just as mountains rising in equatorial lands to the limit of perpetual snow exhibit on their slopes every gradation of climate from tropical to frigid, so the depths of the oceans, as we now know, exhibit a perfectly similar transition. As we go downwards not only heat, but light also, rapidly diminishes, and many forms which, because they came from equatorial regions, we have hitherto regarded as tropical, we now know to live in icy-cold water as well as in almost utter darkness.

The large size and abundant development of Cephalopods, Crustaceans, and fish we now know, from recent deep-sea researches, to be no evidence whatever of the presence either of warmth or of light; and Sir Joseph Hooker has abundantly shown the fallacy of similar reasoning when applied to plant-life. I feel sure that, when the full consequences of these important considerations come to be appreciated, the apparent anomalies of many of the supposed climatal conditions of past geological times will altogether disappear. For my own part, I have never felt any difficulty in accepting, as fully equal to the explanation of the facts of the case, the Lyellian doctrine of climate being determined by great changes in the relative positions of the land and water of the globe.

The other cause of misconception with respect to the conditions which must have prevailed during the deposition of geological deposits consists in the acceptance of an utterly false proposition, which, though seldom formulated, is often tacitly acted upon; namely, "If two organisms exhibit similarity of structure, their environment must have been the same."

There never has been wanting abundant evidence of the fallacy of this doctrine. The general structure of the piscivorous bear of the Arctic regions, and of the frugivorous bear of the Malay peninsula, the osteology of the deer of Lapland and of India respectively, exhibit no such differences as would lead us to infer their diversity of habits and surroundings. It has long been known that elephants, rhinoceroses, and hippopotami, with lions, tigers, and hyænas, flourished under Arctic conditions. The deep-sea researches have so added to our knowledge concerning the conditions under which different forms of life exist—especially those belonging to marine faunas—as to demand a complete reconsideration of the conclusions usually accepted by geologists. For there is a general consensus of opinion among the naturalists who have studied the different groups of the deep-sea faunas, that, contrary to what might have been anticipated from the very remarkable conditions under which they live, the deep-sea forms belong, for the most part, to the same families, and often indeed to the same genera, as shallow-water forms.

The bearing of this important conclusion upon the great problem of the distribution of marine forms of life is obvious. Botanists have naturally availed themselves of the proved occurrences of colder climates in many areas to explain difficult facts of plant-distribution, such as the occurrence of well-known Arctic species on the tops of mountains in what are now temperate, or even tropical, districts. But zoologists, now that they know it to be possible for littoral forms to stray into abysmal portions of the ocean, and then subsequently, without profound modification, to re-emerge in other littoral areas, may find a clue to some very remarkable facts concerning the distribution of marine forms of life, without having to resort to explanations which seem necessary in the case of the terrestrial forms of life which appear to be more dependent than the marine types on the circumstances of their environment.

The whole problem of the distribution of marine forms of life requires indeed to be worked out afresh on the basis of these new discoveries; and when this is done, the first to profit by the new generalizations will be geologists, who have long been confronted by seemingly insuperable difficulties in connection with this problem.

As for the very prevalent notions that Ammonites and Belemnites, Trigoniæ and Brachiopods, with Ichthyosaurs, Pliosaurs, and Plesiosaurs, could only have lived in warm, if not actually tropical, climates, I know of no grounds whatever for any such belief. The nearest living allies of the invertebrates referred to flourish at considerable depths in icy-cold water; and, seeing that large marine mammals now live amid snow and ice, I cannot understand why the great marine reptiles might not have done the same. Just as little reason is there for inferring that Sigillarids, Lepidodendrids, and Calamites could only have lived in tropical jungles, as there is for the once popular notion that they flourished in an atmosphere supplied with a very exceptional proportion of carbonic acid!

The sooner geologists recognize the fact that all our ideas concerning the distribution of the forms of marine life have been completely revolutionized by the discovery that there are cold and dark abysses, which are tenanted by numerous organisms having many affinities with those which live in shallow water, warmed by a tropical sun and flooded with light, the more likely will they be to avoid the errors into which we have fallen in the past. Not until the exact distribution of life-forms at different depths in the ocean has been much more perfectly worked out than it has been at present, will it be safe to reason with any confidence concerning the distribution of extinct types; and, even then, we shall ever have to be on our guard against the prevalent fallacy which assumes that analogies in structure are indicative of similarities in the conditions of life.

And here it may be remarked that the imperfect methods employed on board the *Challenger* and most other surveying ships leave almost everything yet to be done in the way of determining the limits of depth, temperature, pressure, and other conditions under which the different forms of marine life can flourish. It is much to have obtained so great an insight into the characters of some of the creatures inhabiting the deepest parts of the ocean, and of the peculiar conditions which must exist in some of those places where marine life is abundant. But the work which has yet to be done requires the employment of dredges and nets which can be opened when they have reached a certain depth in the ocean, and which can be closed again before being drawn to the surface. Only by the employment of such apparatus can we hope to avoid those sources of error which vitiate all our present generalizations concerning the bathymetrical distribution of the existing forms of marine life.

When, in addition to these biological studies, we have equally careful determinations of the physical characters of deposits formed at varying depths and distances from the shore, and under diverse influences of tides and currents, we may hope, by combining the physical and biological evidence, to arrive at something like certain conclusions concerning the exact conditions under which various geological formations have been accumulated; for at present our speculations upon the subject are often little better than haphazard guesses.

The conditions which must have prevailed during the deposition of a particular bed having been determined, the present mineral condition of the organic remains becomes a subject of very interesting study; for here we may find a clue which will enable us to unravel the series of physical and chemical changes which must have gone on in the mass, since the first accumulation of its materials. In cases of difficulty of this kind, the condition of alteration of a shell or bone, of which the original composition is known, becomes an especially valuable piece of evidence.

I am convinced that the future progress of geological thought is closely bound up with the increase of our knowledge concerning the conditions under which the various forms of marine life flourish, and under which their remains become embedded in sedimentary deposits; though what has been already accomplished in this direction, it must be admitted, is but small, and much of it will have to be done over again.

We hear much—far too much, as I think—at the present day of an "irrational uniformitarianism." Is not the real source of danger in an exactly opposite direction? Does not the irrationality characterize him who, without attempting to obtain a more complete knowledge of the processes going on during the original deposition and subsequent changes of rock-masses, is ready, as each new difficulty presents itself, to fall back upon some old discredited *Deus ex machinâ* in the form of deluges of water, floods of fire, boiling oceans, caustic rains, or acid-laden atmospheres!

Considering how little we as yet know of many of the conditions under which deposits are being formed at the present day, and remembering how large a part of the little we do know has been acquired only within the last few years, we might pause before declaring that the path upon which geology entered in earnest only some fifty years ago is a wrong one, and that the sooner we begin to retrace our steps the better.

Can we even now be in danger of forgetting that "Slough of Despond," wherein the geologist, laden with a grievous burden of traditional assumptions and irrational theories, so hopelessly floundered, till one Help pointed out a way of escape, and sent him on his way rejoicing, with the "Principles of Geology" in his hand?

The second aspect in which palæontological science presents itself to the geologist, is as affording a key to the chronology of the rock-masses of the globe. We still regard fossils as the "medals of creation," and certain types of life we take to be as truly characteristic of definite periods as the coins which bear the image and superscription of a Roman emperor or of a Saxon king.

But in the application of the principle that "strata are to be identified by their organic remains," we have now to admit as many limitations, and to exercise as much caution, as when judging of the conditions under which rock-masses must have been deposited, from the characters of the fossils which they contain.

With the restricted area of the south-west of England, where William Smith achieved his epoch-making discovery, the doctrine which he announced seemed to be absolutely true; each formation exhibited a peculiar and perfectly characteristic assemblage of organic remains, by means of which it could at once be recognized. The still more detailed studies of strata of the same age, by Hunton and Williamson in Yorkshire, by Marcou in the Jura, and by Quenstedt in Swabia, seemed to show that the principle had a wider application than even its author himself could have imagined, and that zones a few feet or even inches in thickness might be followed over considerable districts, everywhere marked by some particular type of Ammonite or other characteristic fossil.

But the more thorough and systematic study of corresponding formations over wide areas, which was inaugurated by Oppel, and has been carried on by many palæontologists since, has abundantly demonstrated that, striking as is the parallelism of the zones in such a formation as the Lias, when studied in England, France, and Germany, yet the species and varieties found on the same horizon at distant points are in many cases not identical, but merely representative; and, further, that as we pass away from any typical area, the sharp distinction between the several zones seems gradually to vanish.

The same facts come out very strikingly when we study any other great geological period. In the oldest fossiliferous strata, those of the Cambrian, nothing can be more striking than the similarity of the faunas in North America, Britain, Scandinavia, and Bohemia; and yet the species which occur at the several different horizons in these countries are certainly, for the most part, not identical, but only representative. No fact, it seems to me, could more clearly indicate that, even at that early period, there were life-provinces with a distribution of organisms in space quite analogous to that which exists at the present day.

To pass to slightly younger rocks. What can be more striking than the evidence of the limits of two life-provinces, afforded by the Calciferous strata of North America and the similar rocks of Scotland and Northern Europe, which contain the remarkable *Maclureas* and a peculiar assemblage of Cephalopods and other fossils; for these are seen at Girvan to come into close contiguity with the more southern type of Silurian, containing a very different fauna, so well seen in the Lake District and North Wales.

Another striking example of the same kind is afforded by the Cretaceous, of which the Southern type, marked by the abundance of *Hippurites*, *Orbitolites*, and other remarkable forms, comes into close relations, as has been so well shown by Hébert, with the type which yields the ordinary Cretaceous fauna of Central Europe. In these and similar cases which might be mentioned we trace the existence of two approximating marine provinces, like those which at the present day are separated by the Isthmus of Panama.

Profs. Neumayr and Mojsisovics have indeed shown that there are good causes for believing that the distinction between the marine zoological provinces in Triassic and Jurassic times was at least as clearly marked as between the similar provinces of the present day; and the former naturalist has in addition pointed out that within the geographical provinces we have also very recognizable climatic zones.

In the year 1862, Prof. Huxley, speaking from this chair, uttered a much-needed warning against the growing practice among palæontologists of treating geological equivalence as meaning the same actual contemporaneity; and against the assumption, without positive proof, that ancient faunas and floras had an indefinite and even world-wide distribution. Palæontological discoveries during the last quarter of a century in Western North America, in India, in the Cape Colony, Australia, and New Zealand, have abundantly justified these cautions, and have shown how much such a term as "homotaxis" is needed, in order to guard against errors resulting from the abuse of the phrase "geological contemporaneity."

But when Prof. Huxley went on to suggest that "a Devonian fauna and flora in the British Isles may have been contemporaneous with Silurian life in North America and with a Carboniferous fauna and flora in Africa," I think that geologists, with the evidence they have now before them, must take exception to so sweeping a generalization. Finding, as we do, on both sides of the Atlantic the same succession of Cambrian, Ordovician, Silurian, Devonian, and Carboniferous strata, containing strikingly representative, if not identical faunas, it is impossible to doubt their general parallelism; however ready we may be to admit that the migration or development of new forms of life in the two areas need not have occurred synchronously, and that thus a certain amount of overlapping of the periods represented at distant points by the same system may exist.

On the other hand, I believe that the study of fossils from remote parts of the earth's surface has abundantly substantiated Prof. Huxley's alternative suggestion that "geographical provinces and zones may have been as distinctly marked in the Palæozoic epoch as at present." The ever-accumulating mass of palæontological evidence seems to me to be all pointing in this direction; and I confidently anticipate that the palæontological anomalies which in the past have caused so much doubt and difficulty, will, by the establishment of this principle, receive a full and satisfactory explanation.

As long ago as 1846, Darwin, in his "Observations on South America," showed that certain assemblages of fossils presented a blending of characters, which in Europe are only found apart in faunas which are of Jurassic and Cretaceous age respectively. Since that date, the study of the fossil faunas and floras of South Africa, India, Australia, New Zealand, and the Western Territories of North America has furnished an abundance of facts of the same kind, showing that no classification of geological periods can possibly be of world-wide application; that we must be contented to study the past history of each great area of the earth's surface independently, and to wait patiently for the evidence which shall enable us to establish a parallelism between the several records. Attempts to establish a universal system of nomenclature or classification of sedimentary rocks are indeed greatly to be deprecated, for if the zoological and botanical distribution of past geological times were at all comparable to that of the present day, any such universal system must be impossible.

The suggestion made to this Society by Prof. Huxley at a somewhat later date is equally valuable and important. Referring to the fauna of the Trias, he said:—"It does not appear to me that there is any necessary relation between the fauna of a given land and that of the seas of its shores. At present our knowledge of the terrestrial faunæ of past epochs is so slight that no practical difficulty arises from using, as we do, sea-reckoning for land-time. But I think it highly probable that, sooner or later, the inhabitants of the land will be found to have a history of their own."

The growth of our knowledge concerning the terrestrial floras and faunas of ancient geological periods, since these words were written in 1869, has constantly forced upon the minds of many geologists the necessity of a duplicate classification of geological periods, based on the study of marine and terrestrial organisms respectively.

Upon this important question the judicious remarks of my colleague, Dr. Blanford, must still be fresh in the minds of all geologists and biologists. He showed that not only are terrestrial provinces independent of marine ones, but that at the present, as well as in the past, the former are more circumscribed and have an amount of distinctness which does not exist in the case of the latter.

Nor is it difficult, in the present state of our biological knowledge, to give a reason for the existence of this state of things. Between completely separated land-areas, migration can only take place by such accidents as the transport of seeds or eggs, or as the consequence of the great but slow changes in the relations of sea and land. Forms adapted only for living in cold climates are isolated by tracts of low-lying tropical land, and, conversely, tropical forms are divided off from one another, by snow-covered mountain-chains, almost as distinctly as by actual oceans. The fact that well-known Arctic plants are found at the top of mountains in tropical or temperate lands, has seemed to many botanists as quite inexplicable without calling in the agency of a general refrigeration, like that which marked the Glacial period.

But with marine forms of life the case is totally different. The oceans are not only much larger than the continents, but they are all more or less completely connected with one another. Forms which live at the surface of the ocean may wander freely in all directions, and know but few limitations except those imposed by temperature, absence of food, &c. ; forms living at moderate depths may migrate along shore-lines or submarine ridges from one area to another; and even when abysmal tracts of ocean intervene between two littoral faunas, recent researches seem to show that the littoral forms of life may wander into such tracts, and eventually, perhaps, cross them, without undergoing extreme or profound modification. In this way, I think, we may account for the important fact so prominently brought into view by Dr. Blanford, that terrestrial life-provinces are and always must have been more restricted in area, and more sharply cut off from one another, than marine provinces.

With the clear recognition of this principle there falls to the ground one of the most frequently urged objections to the uniformitarian doctrines—that, namely, which is based on the supposed differences in geographical distribution in ancient times as compared with the present. I have always doubted whether there is any evidence to show that the marine life-provinces of Silurian or Carboniferous times were of greater extent than those of the present day.

I believe that the doctrine that strata can be identified by the organic remains which they contain is as sound as when it was first enunciated by William Smith ; but the problems of stratigraphical palæontology, as they now present themselves to us, are infinitely more complicated than they could possibly have seemed to him. In every fauna and flora which we are called upon to study, we have to resolve a function of three variables, these being environment, space, and time. Only after the most careful investigation, in the first place, of the complicated effects produced by the varied conditions which we group together under the term environment—temperature, food, absence of enemies, and the innumerable influences which, as we now know, determine the existence and affect the multiplication of living beings ; and by the thorough study, in the second place, of the laws of geographical distribution of plants and animals, can we hope to eliminate the effects due to environment and position, and arrive at the conclusion of what must be ascribed to time.

The task will be long, the work to be done arduous, and the efforts to be made prodigious and sustained ; but the result is one which is not hopeless and unattainable, or, indeed, even doubtful. But let us by all means remember that the real work is really only just commenced, and that we are very far indeed from our goal.

One of our greatest sources of danger to the progress of geological knowledge at the present day is the impatience which is so frequently shown at the rate of that progress, an impatience which leads to attempts to cut the tangled skeins of research by hasty and ill-considered speculation. Geologists, no less than biologists, need to recollect and keep ever before their minds the important fact that the geological record, although it is one of enormous value, is exceedingly imperfect, and that this imperfection is quite as conspicuous in respect to physical as it is to palæontological data. How sadly is this important truth lost sight of by those who, on the strength of a few isolated facts and fragmentary observations, are prepared to construct maps of large portions of the earth's surface at far distant periods of its history. Such maps are to the geologist what "genealogical trees" are to the biologist—"will-o'-the wisps" leading us aside from the safe paths of scientific induction.

It is, I suspect, from the obvious failure of attempts of this kind—attempts which had better never have been made—that such frequent suggestions of revolt against the principles of uniformitarianism take their origin. For myself, instead of disappointment, I feel a constant surprise that these doctrines have enabled us to explain so much, when our knowledge of the causes still at work around us is still so imperfect ; and I am continually impressed by the fact that each new discovery concerning the present order of Nature removes old difficulties in the explanation of the past. In saying that I adhere to the doctrines of uniformitarianism, I, of course, mean the uniformitarianism which Lyell himself taught, and not the absurd travesty of that doctrine sometimes ascribed to him.

The well-grounded conviction which results from observing the triumph of a great principle, when applied in an overwhelming number of cases, and which refuses to abandon that principle at the first appearance of difficulty, is surely not out of place in a student of Nature. It was this scientific "faith" which led Scrope to believe, in spite of difficulties arising from the imperfect knowledge in his day of physics, chemistry, and mineralogy, that massive and schistose crystalline rocks have been formed from ordinary lavas and sediments, when subjected to enormous pressures and complicated earth-movements ; which induced Lyell to seek for and find the key to physical changes during past times in the operations going on everywhere around us ; and which finally conducted Darwin, by the application of the same principle, in the case of living beings, to the doctrine of organic evolution.

But, alas ! this "faith" seems often sadly wanting among us to-day. At a time when the mineralogical constitution of rocks and of the changes which they undergo is becoming daily more clearly revealed, when innumerable researches are throwing fresh light on the great physical processes taking place everywhere in the world around us, and when each department of biological science is contributing new "facts and arguments for Darwin," such scientific pusillanimity on the part of geologists seems, to say the least of it, singularly inopportune.

Doubtless there are difficulties still unresolved ; but does not every advance in our knowledge see the removal of some of them ? True the task of interpreting the fragmentary record of the rocks is one the end of which seems very far off ; but is not every step we take clearly an approximation towards that end ?

If any arguments were needed in favour of the continued and close co-operation of geologists and biologists, it would be found in the circumstance that the most important step in the progress of scientific thought which has been accomplished in modern times has been the direct result of a combination of geological and biological researches.

That remarkable biography, for which we are so greatly indebted to Mr. Francis Darwin, is not simply the record of a life, simple, blameless, and noble beyond that of ordinary men, the story of the workings of an intellect, truth-loving, patient, and powerful, above that of all his contemporaries ; it is the history of a most wonderful revolution in human thought—one which will perhaps be regarded in future times as the most striking event of the nineteenth century.

The grand secret of Darwin's success in grappling with the great problem of "the origin of species" is found in the fact that he was at the same time a geologist and a biologist. The concentration of the later years of his life upon zoological and botanical researches has led many to forget the position occupied by Darwin among geologists. Not only are his geological writings of the highest value for the wealth of accurate observations which they contain, and the important generalizations which they put forward ; but in his more purely biological works the value of his geological training and experience are constantly exemplified.

It was, indeed, a fortunate circumstance that Darwin, after being repelled by the narrow and soulless system of "geognosy" taught by Jameson at Edinburgh, came at Cambridge under the spell of Henslow, a man of most catholic taste, extensive acquirements, and widest sympathy with all branches of natural science. By intercourse with Henslow, Darwin's flagging interest in science was rekindled and kept alive. It is a proud boast for a University to have nourished the intellectual development of Darwin; and as that University has in the past remained faithful to the memory of Newton, making his mathematical teachings the characteristic and leading feature of its studies, so, we may hope, it will in the future aim at that complete union of geological and biological investigation of which Darwin's labours constitute so grand an example.

In the dedication of his "Journal of Researches," Darwin acknowledged "with grateful pleasure" that "the chief part of whatever scientific merit this journal and the other works of the author may possess, has been derived from studying the well-known and admirable 'Principles of Geology';" and well do I recollect how, in almost every conversation I had with him, he would enlarge with warmth of feeling upon his indebtedness to Lyell, not only for his lucid teaching, but for his constant and helpful sympathy. How did he use to speak in terms of reverence of his "master," and extol the magnanimity of one who, though twelve years his senior, had abandoned slowly and cautiously, as was the habit of his mind, yet in the end completely and ungrudgingly, his own conclusions and prepossessions, and had accepted the doctrines of a pupil.

Of Darwin's three geological books, the record of the observations made by him during the voyage of the *Beagle*, it is impossible to speak in terms of praise that will seem, to those acquainted with the merits of those admirable writings, as too high; and some portions of those works, especially the chapters dealing with the great problem of foliation, are, I am convinced, very far indeed from having received from geologists the amount of attention which they deserve.

After Darwin's return to England, in 1836, his attention was for some years almost exclusively devoted to geological researches; and it was to this Society and to its officers that he constantly came for help, advice, and sympathy. He writes at this time, "If I was not more inclined for geology than the other branches of natural history, I am sure Mr. Lyell's and Lonsdale's kindness ought to fix me."

Before reaching England, Darwin had written to Henslow from St. Helena, on July 9, 1836, asking that he might be proposed a Fellow of this Society, and on November 30 of that year he was elected. In the following February he became a member of our Council, and at the next anniversary, in 1838, undertook the duties of Secretary. This office, after he had held it for five years, he was compelled to resign through ill health; but even after he had been driven from London through the same cause, it was the evening meetings of this Society which from time to time tempted him from the seclusion of Down, till at last painful experience proved to him that he must forgo even this too-exciting pleasure. Even after being compelled to lay aside his hammer, when he had taken up scalpel and microscope to study the Cirripedia, he did not forget the fossil forms of the same group.

Whether it was the distribution of organic forms in space, or the order of their appearance in time, which had had most to do in turning Darwin's thoughts into those currents which finally led him to evolution, it would be idle to speculate; but it may safely be asserted that the geological aspects of natural history had at least as much to do with the conception of the origin of species as had the biological.

How warm was Darwin's interest, all through his life, in the progress of every branch of geological research may be gathered from his letters to Lyell and other geological friends. In what he had a presentiment would be, and which actually proved, his latest work, "The Formation of Vegetable Mould through the Action of Worms," he returned in his old age to a geological problem which had occupied him during the years of his most intimate connection with our Society.

No memories can possibly have such fascination for myself as those of the conversations which, during the last seven years of his life, I was privileged to hold with Mr. Darwin upon the current topics of geological interest. It was his habit when he came to town, twice a year, to ask me to meet him, in order to talk over geological questions, and thus I had opportunities for close intercourse and discussion. No researches in our science were too minute, none too remote from the ordinary subjects of his study, to engage his attention and command his sympathies. How keenly did he recall the pleasures of his labours in this Society, and the happiness of the friendships which he had formed here! How generously and with what warmth of appreciation did he ever speak of the labours of those who had succeeded him in endeavouring to carry out the objects of this Society! Of the gentleness, the sympathy, the contagious enthusiasm of the man, I dare not trust myself to speak!

At a time when there is perhaps some danger that the excessive specialization which seems to have become a necessity in both the geological and the biological sciences, may lead to narrowness of view, restriction of aims, and petty jealousies among the workers in circumscribed departments of those sciences, it may be well to remember how Darwin, while engaged in the most minute and detailed investigations upon barnacles, earthworms, or pigeons, upon orchids, primroses, or climbing plants, could ever keep his mind open to the influence of each new discovery in every branch of geological and biological science.

The great principles which lie at the foundation of modern geology and of modern biology are the same; and Darwin did but furnish a new testament to the old covenant already accepted by geologists. Now, more than ever in the history of natural science, is there reason for the warmest sympathy, the most thorough understanding, and the completest union in effort between the cultivators of the geological and the biological sciences. It is not by petulant unfaithfulness to the tried methods of those two sciences, and a readiness to abandon the principles which have led us to such real and important conquests, for the older methods that have been so often discredited and found wanting, that we can hope to advance those sciences.

Lyell once wrote to Darwin as follows: "I really find, when bringing up my preliminary essays in 'Principles' to the science of the present day, so far as I know it, that the great outline, and even most of the details, stand so uninjured, and in many cases they are so much strengthened by new discoveries, especially by yours, that we may begin to hope that the great principles there insisted on will stand the test of new discoveries."

And to this Darwin replied with characteristic enthusiasm:—

"*Begin to hope?* Why the *possibility* of a doubt has never crossed my mind for many a day. This may be very unphilosophical, but my geological salvation is staked upon it! . . . It makes me quite indignant that you should talk of *hoping*."

Fifty years have elapsed since these words were written. How infinitely more complicated seem to us the problems involved in the explanation of the past by the study of the process going on around us at present, than they possibly could have done to the great pioneers of the Uniformitarian doctrines! But the reasons for Lyell's hope and Darwin's confidence are still valid, nay, are stronger than ever. For does not every new discovery remove some difficulty or supply fresh illustrations of these views? May every geologist to-day be endowed with a due share of Lyell's caution; but, for my own part, I see no reason why he should not also possess a full portion of Darwin's faith.

ON THE NUMBER OF DUST PARTICLES IN THE ATMOSPHERE.[1]

AT the beginning of the paper, reference is made to the great advance recently achieved by physiologists, regarding our knowledge of the solid matter floating in the atmosphere, as they have already provided us with a considerable amount of information regarding the number of live germs in the air under different conditions; while we have but little information regarding the dead organic and inorganic particles. The following investigation was undertaken in the hope of bringing the physical side of the subject abreast of the physiological; and in this paper is given an account of a method devised by the author for counting the dust particles in the air, and also some results obtained by means of it.

One difficulty presented in this investigation is the extreme minuteness of the particles to be counted; most of them are

---

[1] Communicated by permission of the Council of the Royal Society of Edinburgh, having been read to the Abstract Society on February 6, by John Aitken, F.R.S.E.